the markings of particular parts or areas. Generally, it is not only this, but color differences combined with variations either in general size or in the size of special parts or organs, in which the variations of different parts are sometimes in opposite directions. If the author had worked directly from large series of specimens, instead of taking data tabulated by others relating to the single character of size, it is pretty safe to say that the paper here under notice would not have been written.

In the case of *Zapus* there is no reason for doubt in respect to the status of the two forms. They present as clear and well pronounced evidence of specific distinctness as could well be looked for between congeneric forms. In the case of *Scalops* the curves of differentiation are based on the single character of general size, the length of the skull being taken as the basis. Other characters of perhaps equal or even greater importance, as the increase in the size of the teeth with decrease in skull length or in general size, the relative length of the tail and marked differences in color are ignored, perhaps because the differences in these features are not easily reducible to 'quantitative expressions.' But taking size alone, what kind of a 'method' is it that attempts to determine quantitative difference, say between *Scalops aquaticus* from Massachusetts, Connecticut and New York and *Scalops machrinus* from Minnesota, Illinois and Iowa by taking in the first case a few specimens at irregular and infrequent intervals from Cape Cod to Charleston, S. C., and in the other in a similar way from Minnesota to Louisiana? In either case the difference in size is greater between specimens from the northern and the southern points in either series than between specimens from corresponding points in latitude between *S. aquaticus* and *S. machrinus!* Mr. Davenport's Fig. 8 thus shows nothing of any value whatever. The quantitative study of variation is a problem of great interest and importance, but this is not the way to go about it. The ideal way, and one which would be profitable in results, would be to take a sufficiently large series of adult specimens, say in the case of *Scalops* of not less than 20 to 50 from judiciously selected localities not more than 100 miles apart, along at least two lines, the one meridianal, the other on a parallel of latitude (due regard in each case being had for differences of elevation), and subject each available character to quantitative analysis. Were this done on a series of such intersecting lines extending throughout the ranges of all the forms of a genus the results might then be expressed in curves that would reflect actual facts and throw important light on the status and real relationships of all the forms involved. It might be well worth doing, at least in the case of a few groups, for the general bearing such results would have on the problems of evolution; but the millenium of a precise knowledge of species and subspecies for any class of animals—say of North American mammals—will not arrive in our day if we must wait for the production of that delightful result by the process of quantitative determination of character variation. The work and expense involved is too great, and long before final results would be available the methods now in vogue of studying comparatively large series from as many localities as possible will probably have already covered and decided most of the points such an elaborate system might be properly expected to establish.

J. A. ALLEN.

A NECESSARY CORRECTION.

TO THE EDITOR OF SCIENCE: In an article claiming to be a review of 'The Living Substance' (Supplement to *Journal of Morphology*), which appeared in *Nature* recently, the reviewer, F. A. D., says: "*The authoress of this wordy treatise informs us (p. 173) that she started from a neutral position with regard to Bütschli's vesicular theory, or even with a bias against it. Now, however, having become the most ardent of converts, she proceeds, with the proverbial zeal of a proselyte, to carry the original doctrine to extremes. Not content with proclaiming the existence of foams undreamt of by Bütschli—'wheels within wheels' ad infinitum—she utters what amounts to a denunciation of all previous statements of biological fact and theory as misleading and inadequate, and urges in effect that the whole science of life needs recasting from the new point of view.*"

Now, Bütschli's famous vesicular theory of protoplasm argues for a physico-chemical interpretation of protoplasmic activities. Believing that he had discovered a fundamental vesicular structure, Bütschli held that amœboid movements, the phenomena of cell division, and even contractility, may be interpreted as results of osmotic interchanges, surface tensions and extension currents amongst the lifeless lamellæ of this structure. He admits, however, "I find myself unable * * to apply the same physical explanation to the finer formations, such as the free filose formations * *." Further, that "the morphological method, so fruitful in research amongst multicellular organisms, fails in our research into the essential nature of the elementary organism—the cell."

Compare the following from 'The Living Substance.'

(1) "The vital phenomena of protoplasm were seen to be not so much manifestations of the vesicular form of the substance, as upon, or through, this," p. 67. (2) "The co-existence of a stable and perfect structure of Bütschli, with a host of metamorphosing activities of the substance as such; this forms one of the strongest reasons I would urge for preferring optical research upon the living material," p. 68. (3) "It is the free filose formations, not amœboid flow, which I find to be most universal, most characteristic and most fundamental in the living substance," p. 78. (4) "It is then, not that compound of cells whose multiplication we have watched with such breathless interest that is the true organism, but the continuous substance, by whose local deposits of specific materials these cells and their nuclear machinery are built up * * * . And within the organism's limits, the protoplastic substance as such retains, one must now believe, all those protoplastic powers which are seen in free Heliozoa—all those tactile and selective and sensitively irritable, and contractile, functions that protoplasm exhibits when placed externally to cells, areas, or masses. On this protoplastic substance the race habit depends, and in it are rooted all other habits of organisms," p. 170. (5) "Organs no longer appear as compounds of certain different sorts of cells, but as a complex of minute substance organs," p. 151. (6) "The organism as we have known it, is secondary, incidental to the life-history of the protoplastic, continuous substance of the living being; is result, rather than cause, of substance habit," p. 171. (7) "We are not denied an ultimate return to purely physical interpretations * * * but we are bidden for the time to a physiological standpoint as more immediately fruitful," p. 119. Under Areal Differentiation, Striation and Activities, hundreds of radically new facts prove that any present application of physico-chemical explanations, by means of vesicular structure, to either contractility or cell division is not possible.

The rest of F. A. D's article bears out the beginning, is a crescendo of similar blunders and guesses—personal in tone, hysteric in timbre, and unsupported by a single quotation; but in view of the cardinal absurdity of wholly ignoring the text of the book 'criticised,' one's sense of humor vetoes further analysis.

G. F. ANDREWS.

SCIENTIFIC LITERATURE.

La face de la terre (Das Anlitz der Erde). EDOUARD SUESS. Translated from the German with the approval of the author and annotated under the direction of EMM. DE MARGERIE, with a preface by PROFESSOR MARCEL BERTRAND. Paris, Armand Colin et Cie. 1897. Vol. I. Pp. 835. With two maps in colors and 122 figures.

The first part of *Das Anlitz der Erde* appeared in 1895. The author set himself the task of marshalling the movements of the earth's crust into a system. The work gives the result of his studies of mountain systems and of the adjacent plateaus and plains. From its scope and the radical views of the author, the treatise takes a place in geological literature with the famous Notices sur les systems des montagnes of De Beaumont, published in 1852.

De Beaumont gave us, perhaps, the first clear statement of the contraction hypothesis in its relation to mountain building. In his treatise on mountains he sought to establish the principle that mountain chains of the same age are parallel to the same great circle. In attempting to defend his thesis, De Beaumont made